SCIENCE

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ADDRESS OF THE PRESIDENT TO THE PHYSIOLOGICAL SECTION OF THE BRITISH ASSOCIATION.

WE who have come from the little island on the other side of the great waters to take part in this important gathering of the British Association have of late been much exercised in retrospection. We have been looking back on the sixty years' reign of our beloved Sovereign, and dwelling on what has happened during her gracious rule. We have, perhaps, done little in calling to mind the wrongs, the mistakes and the failures of the Victorian era; but our minds and our mouths have been full of its achievements and its progress; and each of us, of himself or through another, has been busy in bringing back to the present the events of more than half a century of the past. It was while I, with others, was in this retrospective mood that the duty of preparing some few words to say to you to-day seemed suddenly to change from an impalpable cloud in the far distance to a heavy burden pressing directly on the back; and in choosing something to say I have succumbed to the dominant influence. Before putting pen to paper, however, I recovered sufficiently to resist the temptation to add one more to the many reviews which have appeared of the progress of physiology during the Victorian era. I also rejected the idea of doing that for which I find precedents in past presidential addresses—namely, of at-

tempting to tell what has been the history of the science to which a Section is devoted during the brief interval which has elapsed since the Section last met; to try and catch physiology, or any other science, as it rushes through the brief period of some twelve months seemed to me not unlike photographing the flying bullet without adequate apparatus; the result could only be either a blurred or a delusive image. But I bethought me that this is not the first, we hope it will not be the last, time that the British Association has met in the Western Hemisphere; and though the events of the thirteen years which have slipped by since the meeting at Montreal in 1884 might seem to furnish a very slender oat on which to pipe a presidential address, I have hoped that I might be led to sound upon it some few notes which might be listened to.

And indeed, though perhaps when we come to look into it closely almost every period would seem to have a value of its own, the past thirteen years do, in a certain sense, mark a break between the physiology of the past and that of the future. When the Association met at Montreal in 1884, Darwin, whose pregnant ideas have swayed physiology in the limited sense of that word, as well as that broader study of living beings which we sometimes call biology, as indeed they have every branch of natural knowledge, had been taken from us only some two years before, and there were still alive most of the men who did the great works of physiology of the middle and latter half of this century. The gifted Claude Bernard had passed away some years before, but his peers might have been present at Montreal. Bowman, whose classic works on muscle and kidney stand out as peaks in the physiological landscape of the past, models of researches finished and complete so far as the opportunities of the time would allow, fruitful beginnings and admirable guides for the labors of others. Brown-Sequard, who shares with Bernard the glory of having opened up the great modern path of the influence of the nervous system on vascular and thus on nutritional events, and who, if he made some mistakes, did many things which will last for all time. Brücke, whose clear judgment, as shown in his digestive and other work, gave permanent value to whatever he put forth. Du Bois-Reymond, who, if he labored in a narrow path, set a brilliant example of the way in which exact physical analysis may be applied to the phenomena of living beings, and in other ways had a powerful influence on the progress of physiology. Donders, whose mind seemed to have caught something of the better qualities of the physiological organ to which his professional life was devoted, and our knowledge of which he so largely extended, so sharply did he focus his mental eye on every physiological problem to which he turned—and these were many and varied. Helmholtz, whose great works on vision and hearing, to say nothing of his earlier distinctly physiological researches, make us feel that if physics gained much, physiology lost even more when the physiologist turned aside to more distinctly physical inquiries. Lastly, and not least, Ludwig, who by his own hands or through his pupils did so much to make physiology the exact science which it is to-day, but which it was not when he began his work. I say lastly, but I might add the name of one who, though barred by circumstances from contributing much directly to physiology by way of research, so used his powerful influence in many ways in aid of physiological interests as to have helped the science onward to no mean extent, at least among English-speaking people—I mean Huxley. All these might have met at Montreal. They have all left us now. Among the peers of the men I have mentioned whose chief labors were carried on in the forties, the fifties

and the sixties of the century, one prominent inquirer alone seems to be left, Albert von Kölliker, who in his old age is doing work of which even he in his youth might have been proud. The thirteen years which have swept the others away seem to mark a gulf between the physiological world of to-day and that of the time in which most of their work was done.

They are gone, but they have left behind their work and their names. May they of the future, as I believe we of the present are doing, take up their work and their example, doing work other than theirs but after their pattern, following in their steps.

In the thirteen years during which these have passed away physiology has not been idle. Indeed, the more we look into the period the more it seems to contain.

The study of physiology, as of other sciences, though it may be stimulated by difficulties (and physiology has the stimulus of a special form of opposition unknown to other sciences), expands under the sunshine of opportunity and aid. And it may be worth while to compare the opportunities for study of physiology in 1884 with those in 1897. At this meeting of the British Association I may fitly confine myself, I was going to say, to British matters; but I feel at this point, as others have felt, the want of a suitable nomenclature. We who are gathered here to-day have, with the exception of a few honored guests from the Eastern Hemisphere, one common bond, one common token of unity, and, so far as I know, one only; I am speaking now of outward tokens; down deeper in our nature there are, I trust, yet others. We all speak the English tongue. Some of us belong to what is called Great Britain and Ireland, others to that which is sometimes spoken of as Greater Britain. But there are others here who belong to neither; though English in tongue, they are in no sense British. To myself, to whom the being English in speech is a fact of far deeper moment than any political boundary, and who wish at the present moment to deal with the study of physiology among all those who speak the English tongue, there comes the great want of some word which will denote all such. I hope, indeed I think, that others feel the same want too. The term Anglo-Saxon is at once pedantic and incorrect, and yet there is none other; and, in the absence of such a better term, I shall be forgiven if I venture at times to use the seemingly narrow word English as really meaning something much broader than British in its very broadest sense.

Using English in this sense, I may, I think, venture to say that the thirteen years which separate 1884 from to-day have witnessed among English people a development of opportunities for physiological study such as no other like period has seen. It is not without significance that only a year or two previous to this period, in England proper, in little England, neither of the ancient Universities of Oxford and Cambridge, which, historically at least, represent the fullest academical aspirations of the nation, possessed a chair of physiology; the present professors, who are the first, were both appointed in 1883. Up to that time the science of physiology had not been deemed worthy, by either university, of a distinctive professorial mechanism. The act of these ancient institutions was only a manifestation of modern impulses, shared also by the metropolis and by the provinces at large. Whereas up to that time the posts for teaching physiology, by whatever name they were called, had been in most cases held by men whose intellectual loins were girded for other purposes than physiology, and who used the posts as stepping-stones for what they considered better things, since that time, as each post became vacant, it had almost invariably been filled by men wishing and pur-

posing at least to devote their whole energies to the science. Scotland, in many respects the forerunner of England in intellectual matters, had not so much need of change; but she, too, has moved in the same direction, as has also the sister island.

And if we turn to this Western Continent, we find in Canada and in the States the same notable enlargement of physiological opportunity, or even a still more notable one. If the English-speaking physiologist dots on the map each place on this Western Hemisphere which is an academic focus of his science, he may well be proud of the opportunities now afforded for the development of English physiology; and the greater part of this has come within the last thirteen years.

Professorial chairs or their analogues are, however, after all but a small part of the provision for the development of physiological science. The heart of physiology is the laboratory. It is this which sends the life-blood through the frame; and in respect to this, perhaps, more than to anything else, has the progress of the past thirteen years been striking. Doubtless, on both sides of the waters there were physiological laboratories, and good ones, in 1884; but how much have even these during that period been enlarged and improved, and how many new ones have been added? In how many places, even right up to about 1884, the professor or lecturer was fain to be content with mere lecture experiments and a simple course of histology, with perhaps a few chemical exercises for his students! Now each teacher, however modest his post, feels and says that the authorities under whom he works are bound to provide him with the means of leading his students along the only path by which the science can be truly entered upon, that by which each learner repeats for himself the fundamental observations on which the science is based.

But there is a still larger outcome from the professorial chair and the physiological laboratory than the training of the student; these are opportunities not for teaching only, but also for research. And perhaps in no respect has the development during the past thirteen years been so marked as in this. Never so clearly as during this period has it become recognized that each post for teaching is no less a post for learning, that among academic duties the making knowledge is as urgent as the distributing it, and that among professorial qualifications the gift of garnering in new truths is at least as needful as facility in the didactic exposition of old ones. Thirteen years has seen a great change in this matter, and the progress has been perhaps greater on this side of the water than on the other, so far as English-speaking people are concerned. We on the other side have witnessed with envy the establishment on this side of a university, physiology having in it an honored place, the keynote of which is the development of original research. It will, I venture to think, be considered a strong confirmation of my present theme that the Clark University at Worcester was founded only ten years ago.

And here, as an English-speaking person, may I be allowed to point out, not without pride, that these thirteen years of increased opportunity have been thirteen years of increased fruitfulness. In the history of our science, among the names of the great men who have made epochs, English names, from Harvey onwards, occupy no mean place; but the greatness of such great men is of no national birth; it comes as it lists, and is independent of time and of place. If we turn to the more everyday workers, whose continued labors more slowly build up the growing edifice and provide the needful nourishment for the greatness of which I have just spoken, we may, I will dare to say, affirm that the last thirteen

years has brought contributions to physiology, made known in the English tongue, which, whether we regard their quanity or their quality, significantly outdo the like contributions made in any foregoing period of the same length. Those contributions have been equally as numerous, equally as good on this side as on the other side of the waters. And here I trust I shall be pardoned if personal ties and affection lead me to throw in a personal word. May I not say that much which has been done on this side has been directly or indirectly the outcome of the energy and gifts of one whom I may fitly name on an occasion such as this, since, though he belonged to the other side, his physiological life was passed and his work was done on this side, one who has been taken from us since this Association last met, Henry Newell Martin?

Yes, during these thirteen years, if we put aside the loss of comrades, physiology has been prosperous with us and the outlook is bright; but, as every cloud has its silver lining, so shadow follows all sunshine, success brings danger, and something bitter rises up amid the sweet of prosperity. The development of which I have spoken is an outcome of the progressive activity of the age, and the dominant note of that activity is heard in the word 'commercial.' Noblemen and noblewomen open shop, and every one, low as well as high, presses forward towards large or quick profits. The very influences which have made devotion to scientific inquiry a possible means of livelihood, and so fostered scientific investigation, are creating a new danger. The path of the professor was in old times narrow and strait, and only the few who had a real call cared to tread it; nowadays there is some fear lest it becomes so broad and so easy as to tempt those who are in no way fitted for it. There is an increasing risk of men undertaking a research, not because a question is crying out to them to be answered, but in the hope that the publication of their results may win for them a lucrative post. There is, moreover, an even greater evil ahead. The man who lights on a new scientific method holds the key of a chamber in which much gold may be stored up; and strong is the temptation for him to keep the new knowledge to himself until he has filled his fill, while all the time his brother-inquirers are wandering about in the dark through lack of that which he possesses. Such a selfish withholding of new scientific truth is beginning to be not rare in some branches of knowledge. May it never come near us!

Now I will, with your permission, cease to sound the provincial note, and ask your attention for a few minutes while I attempt to dwell on what seem to me to be some of the salient features of the fruits of physiological activity, not among English-speaking people only, but among all folk, during the past thirteen years.

When we review the records of research and discovery over any lengthened period, we find that in every branch of the study progress is irregular, that it ebbs and flows. At one time a particular problem occupies much attention, the periodicals are full of memoirs about it, and many of the young bloods flash their maiden swords upon it. Then again for a while it seems to lie dormant and unheeded. But quite irrespective of this feature, which seems to belong to all lines of inquiry, we may recognize two kinds of progress. On the one hand, in such a period, in spite of the waves just mentioned, a steady advance continually goes on in researches which were begun and pushed forward in former periods, some of them being of very old date. On the other hand, new lines of investigation, starting with quite new ideas or rendered possible by the introduction of new methods, are or may be begun. Such nat-

urally attract great attention, and give a special character to the period.

In the past thirteen years we may recognize both these kinds of progress. Of the former kind I might take, as an example, the time-honored problems of the mechanics of the circulation. In spite of the labor which has been spent on these in times of old, something always remains to be done, and the last thirteen years have not been idle. The researches of Hürthle and Tigerstedt, of Roy and Adami, not to mention others, have left us wiser than we were before. So again, with the also old problems of muscular contraction, progress, if not exciting, has been real; we are some steps measurably nearer understanding what is the exact nature of the fundamental changes which bring about contraction and what are the relations of those changes to the structure of muscular fibre. In respect to another old problem, too, the beat of the heart, we have continued to creep nearer and nearer to the full light. Problems again, the method of attacking which is of more recent origin, such as the nature of secretion, and the allied problem of the nature of transudation, have engaged attention and brought about that stirring of the waters of controversy which, whatever be its effects in other departments of life, is never in science wholly a waste of time, if indeed it be a waste of time at all, since, in matters of science, the tribunal to which the combatants of both sides appeal is always sure to give a true judgment in the end. In the controversy thus arisen, the last word has perhaps not yet been said, but whether we tend at present to side with Heidenhain, who has continued into the past thirteen years the brilliant labors which were, perhaps, the distinguishing features of physiological progress in preceding periods, and who in his present sufferings carries with him, I am sure, the sympathies, if not the hopes, of all his brethren, or whether we are more inclined to join those who hold different views, we may all agree in saying that we have, in 1897, distinctly clearer ideas of why secretion gathers in an alveolus or lymph in a lymph space than we had in 1884.

I might multiply such examples of progress on more or less old lines until I wearied you; but I will try not to do so. I wish rather to dwell for a few minutes on some of what seems to be the salient new features of the period under review.

One such feature is, I venture to think, the development of what may perhaps be called the new physiological chemistry. We always are, and for a long time have been, learning something new about the chemical phenomena of living beings. During the years preceding those immediately recent, great progress, for which we have especially, perhaps, to thank Kühne, was made in our knowledge of the bodies which we speak of as proteids and their allies. But while admitting to the full the high value of all these researches, and the great light which they threw on many of the obscurer problems of the chemical changes of the body, such, for instance, as the digestive changes and the clotting of blood, it could not but be felt that their range was restricted and their value limited. Granting the extreme usefulness of being able to distinguish bodies though their solution or precipitation by means of this or that salt or acid, this did not seem to promise to throw much light on the all-important problem as to what was the connection between the chemical constitution of such bodies and their work in the economy of a living being. For it need not be argued that this is an all-important problem. To-day, as yesterday and in the days before, the mention of the word vitalism or its equivalent separates as a war-cry physiologists into two camps, one contending that all the phenomena of life can, and

the other that they cannot, be explained as the result of the action of chemico-physical forces. For myself, I have always felt that while such a controversy, like other controversies as I ventured to say just now, is useful as a stirring of the waters, through which much oxygen is brought home to many things and no little purification effected, the time for the final judgment on the question will not come until we shall more clearly understand than we do at present what we mean by physical and chemical, and may, perhaps, be put off until somewhere near the end of all things, when we shall know as fully as we ever shall what the forces to which we give these names can do and what they cannot. Meanwhile, the great thing is to push forward, so far as may be, the chemical analysis of the phenomena presented by living beings. Hitherto the physiological chemists, or the chemical physiologists as perhaps they ought rather to be called, have perhaps gone too much their own gait, and have seemed to be constructing too much a kind of chemistry of their own. But that, may I say, has in part been so because they did not receive from their distinctly chemical brethren the help of which they were in need. May I go so far as to say that to us physiologists these our brethren seemed to be lagging somewhat behind, at least along those lines of their science which directly told on our inquiries? That is, however, no longer the case. They are producing work and giving us ideas which we can carry straight into physiological problems. The remarkable work of Emil Fischer on sugars, one of the bright results of my period of thirteen years, may fully be regarded as opening up a new era in the physiology of the carbohydrates; opening up a new era because it has shown us the way how to invesigate physiological problems on purely and distinctively chemical lines. Not in the carbohydrates only, but in all directions our younger investigators are treating the old problems by the new chemical methods; the old physiological chemistry is passing away; nowhere, perhaps, is the outlook more promising than in this direction; and we may at any time receive the news that the stubborn old fortress of the proteids has succumbed to the new attack.

Another marked feature of the period has been the increasing attention given to the study of the lower forms of life, using their simpler structures and more diffuse phenomena to elucidate the more general properties of living matter. During the greater part of the present century physiologists have, as a rule, chosen as subjects of their observations almost exclusively the vertebrata; by far the larger part of the results obtained during this time have been gained by inquiries restricted to some half a dozen kinds of backboned animals; the frog and the myograph, the dog and the kymograph, have almost seemed the alpha and the omega of the science. This has been made a reproach by some, but, I cannot help thinking, unjustly. Physiology is, in its broad meaning, the unravelling of the potentialities of things in the conditions which we call living. In the higher animals the evolution by differentiation has brought these potentialities, so to speak, near the surface, or even laid them bare as actual properties capable of being grasped. In the lower animals they still lie deep buried in primeval sameness; and we may grope among them in vain unless we have a clue furnished by the study of the higher animal. This truth seems to have been early recognized during the progress of the science. In the old time, observers such as Spallanzani, with but a moderate amount of accumulated knowledge behind them, and a host of problems before them, with but few lines of inquiry as yet definitely laid down, were free to choose the subjects of their investigation where they pleased, and in the

wide field open to them prodded, so to speak, among all living things, indifferent whether they possessed a backbone or no. But it soon became obvious that the study of the special problems of the more highly organized creature was more fruitful, or at least more easily fruitful, than that of the general problems of the simpler forms; and hence it came about that inquiry, as it went on, grew more and more limited to the former. But an increasing knowledge of the laws of life as exemplified in the differentiated phenomena of the mammal is increasingly fitting us for a successful attack on the more general phenomena of the lowly creatures possessing little more than that molecular organization, if such a phrase be permitted, which alone supplies the conditions for the manifestation of vital activities. And, though it may be true that in all periods men have from time to time labored at this theme, I think that I am not wrong in saying that the last dozen years or so mark a distinct departure both as regards the number of researches directed to it, and also, what is of greater moment, as regards the definiteness and clearness of the results thereby obtained. One has only to look at the results recorded in the valuable treatises of Verworn and Biedermann, whether obtained by the authors themselves or by others, to feel great hope that in the immediately near future a notable advance will be made in our grasp of the nature of that varying collection of molecular conditions, potencies and changes, slimy hitherto to the intellectual no less than to the physical touch, which we are in the habit of denoting by the more or less magical word protoplasm. And, perhaps, one happy feature of such an advance will be one step in the way of that reintegration which men of science fondly hope may ultimately follow the differentiation of studies now so fierce and attended by many ills; in the problems of protoplasm the animal physiologist touches hands with the botanist, and both find that under different names they are striving towards the same end.

Closely allied to and, indeed, a part of the above line of inquiry is the study of the physiological attributes of the cell and of their connection with its intrinsic organization. This is a study which, during the last dozen years, has borne no mean fruits; but it is an old study, one which has been worked at from time to time, reviving again and again as new methods offered new opportunities. Moreover, it will probably come directly before us in our sectional work, and, therefore, I will say nothing more of it here.

Still another striking feature of the past dozen years has been the advance of our knowledge in regard to those events of the animal body which we have now learned to speak of as 'internal secretion.' This knowledge did not begin in this period. The first note was sounded long ago in the middle of the century, when Claude Bernard made known what he called 'the glycogenic function of the liver.' Men, too, were busy with the thyroid body and the suprarenal capsules long before the meeting of the British Association at Montreal. But it was since then, namely in 1889, that Minkowski published his discovery of the diabetic phenomena resulting from the total removal of the pancreas. That, I venture to think, was of momentous value, not only as a valuable discovery in itself, but especially, perhaps, in confirming and fixing our ideas as to internal secretion, and in encouraging further research.

Minkowski's investigation possessed this notable feature, that it was clear, sharp and decided, and, moreover, the chief factor, namely sugar, was subject to quantitative methods. The results of removing the thyroid body had been to a large extent general, often vague, and in some cases uncertain; so much so as to justify, to a cer-

tain extent, the doubts held by some as to the validity of the conclusion that the symptoms witnessed were really and simply due to the absence of the organ removed. The observer who removes the pancreas has to deal with a tangible and measurable result, the appearance of sugar in the urine. About this there can be no mistake, no uncertainty. And the confidence thus engendered in the conclusion that the pancreas, besides secreting the pancreatic juice, effects some notable change in the blood passing through it, spread to the analogous conclusions concerning the thyroid and the suprarenal, and moreover suggested further experimental inquiry. By those inquiries all previous doubts have been removed; it is not now a question whether or no the thyroid carries on a so-called internal secretion; the problem is reduced to finding out what it exactly does and how exactly it does it. Moreover, no one can at the present day suppose that this feature of internal secretion is confined to the thyroid, the suprarenal and the pancreas; it needs no spirit of prophecy to foretell that the coming years will add to physiological science a large and long chapter, the first marked distinctive verses of which belong to the dozen years which have just passed away.

The above three lines of advance are of themselves enough to justify a certain pride on the part of the physiologist as to the share which his science is taking in the forward movements of the time. And yet I venture to think that each and all of these is wholly overshadowed by researches of another kind, through which knowledge has made, during the past dozen years or so, a bound so momentous and so far-reaching that all other results gathered in during the time seem to shrink into relative insignificance.

It was a little before my period, in the year 1879, that Golgi published his modest note, 'Un nuovo processo di technica microscopia.'* That was the breaking out from the rocks of a little stream which has since swollen into a great flood. It is quite true that long before a new era in our knowledge of the central nervous system had been opened up by the works of Ferrier and of Fritch and Hitzig. Between 1870 and 1880 progress in this branch of physiology had been continued and rapid. Yet that progress had left much to be desired. On the one hand, the experimental inquiries, even when they were carried out with the safeguard of an adequate psychical analysis of the phenomena which presented themselves, and this was not always the case, sounded a very uncertain note, at least when they dealt with other than simply motor effects. They were, moreover, not unfrequently in discord with clinical experience. In general the conclusions which were arrived at through them, save such as were based on the production of easily recognized and often measurable movements, were regarded by many as conclusions of the kind which could not be ignored, which demanded respectful attention, and yet which failed to carry conviction. It seemed to be risking too much to trust too implicitly to the apparent teaching of the results arrived at; something appeared wanting to give these their full validity, to explain their full and certain meaning by showing their connection with what was known in other ways and by other methods. On the other hand, during nearly all this time, in spite of the valuable results acquired by the continually improving histological technique, by the degeneration method and by the developmental method, by the study of the periods of myelination, most of us, at all events, were sitting down, as our forefathers had done, before the intricate maze of encephalic structure, fascinated by its complexity, but

**Rendiconti del reale Istitulo Lombardo*, Vol. XII., p. 206.

wondering what it all meant. Even when we attempted to thread our way through the relatively simple tangle of the spinal cord, to expect that we should ever see our way so to unravel out the strands of fibres, here thick, there thin, now twisting and turning, and anon running straight, or so to set out in definite constellations the seeming milky way of star-like cells, so to do this as to make the conformation of the cord explain the performances of which it is capable, appeared to be something beyond our reach. And when we passed from the cord to those cerebral structures the even gross topography of which is the despair of the beginner in anatomical studies, the multiple maze of gray and white matter seemed to frame itself into the letters graven on the gateway of the city of Dis, and bid us leave all hope behind.

What a change has come upon us during the past dozen years, and how great is the hope of ultimate success which we have to-day. Into what at the meeting at Montreal seemed a cloudy masss, in which most things were indistinct and doubtful, and into which each man could read images of possible mechanisms according as his fancy led, the method of Golgi has fallen like a clarifying drop, and at the present moment we are watching with interest and delight how that vague cloud is beginning to clear up and develop into a sharp and definite picture, in which lines objectively distinct and saying one thing only reveal themselves more and more. This is not the place to enter into details, and I will content myself with pointing out as illustrative of my theme the progress which is being made in our knowledge of how we hear and how sounds effect us. A dozen years ago we possessed experimental and clinical evidence which led us to believe that auditory impulses sweeping up the auditory nerve became developed into auditory sensations through events taking place in the temporo-sphenoidal convolution, and we had some indications that as these passed upward through the lower and middle brain the striæ acusticæ and the lateral fillet had some part to play. Beyond this we knew but little. To-day we can with confidence construct a diagram which he who runs can read, showing how the impulses undergoing a relay in the tuberculum acusticum and accessory nucleus pass by the striæ acusticæ and trapezoid fibres to the superior olive and trapezoid nucleus, and onwards by the lateral fillet to the posterior corpus quadrageminum and to the cortex of the temporo-sphenoidal convolution. And if much, very much, yet remains to be done even in tracking out yet more exactly the path pursued by the impulses, while they are yet still undeveloped impulses, not as yet lit up with consciousness, and in understanding the functional meaning of relays and apparently alternate routes, to say nothing of the deeper problems of when and how the psychical element intervenes, we feel that we have in our hands the clue by means of which we may hope to trace out clearly the mechanisms by which, whether consciousness plays its part or no, sounds affect so profoundly and so diversely the movements of the body, and haply some time or other to tell, in a plain and exact way, the story of how we hear. I have thus referred to hearing because the problems connected with this seemed, thirteen years ago, so eminently obscure; it appeared so preeminently hard a task, that of tracing out the path of an ordinary impulse through the confused maze of fibre and cell presented by the lower and middlebrain. Of the mechanism of sight we seemed even then to have better knowledge, but how much more clearly do we, so to speak, see vision now? So also with all other sensations, even those most obscure ones of touch and pain; indeed, all over the nervous system light seems breaking in a most remarkable way.

This great and significant progress we owe, I venture to say, to Golgi, to the method introduced by him; and I for one cannot help being glad that this important contribution to science, as well as another contingent and most valuable one, the degeneration method of Marchi, should be among the many tokens that Italy, the mother of all sciences in times gone by, is now once more taking her right place in scientific no less than in political life. We owe, I say, this progress to Golgi in the sense that the method introduced by him was the beginning of the new researches. We owe, moreover, to Golgi not the mere technical introduction of the method, but something more. He himself pointed out the theoretical significance of the results which his method produced; and if in this he has been outstripped and even corrected by others, his original merit must not be allowed to be forgotten. Those others are many, in many lands. The first, perhaps, was Frithiof Nansen, whose brief but brilliant memoir makes us selfish physiologists regret that the icy charms of the North Pole so early froze in him the bubbling springs of histological research. Of the rest two names stand out conspicuous. If rejuvenescent Italy invented this method, another ancient country, whose fame, once brilliant in the past, like that of Italy, suffered in later times an eclipse, produced the man who, above all others, has showed us how to use it. At the meeting at Montreal a voice from Spain telling of things physiological would have seemed a voice crying out of the wilderness; to-day the name of Ramon-y-Cayal is in every physiologist's mouth. That is one name, but there is yet another. Years ago, when those of us who are now veterans and see signs that it is time for us to stand aside were spelling out the primer of histology, one name was always before us as that of a man who touched every tissue and touched each well. It is a consoling thought to some of the elder ones that histological research seems to be an antidote to senile decay. As the companion of the young Spaniard in the pregnant work on the histology of the central nervous system done in the eighties and the nineties of the century, must be named the name of the man who was brilliant in the fifties, Albert von Kölliker.

When I say that the progress of our knowledge of the central nervous system during the past thirteen years has been largely due to the application of the method of Golgi, I do not mean that it, alone and by itself, has done what has been done. That is not the way of science. Almost every thrust forward in science is a resultant of concurrent forces working along different lines; and in most cases at least significant progress comes when efforts from different quarters meet and join hands. And especially as regards methods it is true that their value and effect depend on their coming at their allotted times. As I said above, neither experimental investigation nor clinical observation nor histological inquiry by the then known methods had been idle before 1880. They had, moreover, borne even notable fruits, but one thing was lacking for their fuller fruition. The experimental and clinical results all postulated the existence of clear definite paths for impulses within the central nervous system, of paths moreover which, while clear and sharp, were manifold and, under certain conditions, alternate or even vicarious, and were so constructed that the impulses as they swept along them underwent from time to time—that is, at some place or other—transformations or at least changes in nature. But the methods of histological investigations available before that of Golgi, though they taught us much, failed to furnish such an analysis of the tangle of gray and white matter as would clearly indicate the paths required. This

the method of Golgi did, or rather is doing. Where gold failed silver has succeeded, and is succeeding. Thanks to the black tract which silver when handled in a certain way leaves behind it in the animal body, as indeed it does elsewhere, we can now trace out, within the central nervous system, the pathway afforded by the nerve cell and the nerve cell alone. We see its dendrites branching out in various directions, each alert to dance the molecular dance assigned to it at once by the more lasting conditions which we call structural, and the more passing ones which we call functional, so soon as some partner touch its hand. We see the body of the cell with its dominant nucleus ready to obey and yet to marshal and command the figure so started. We see the neuraxon prepared to carry that figure along itself, it may be to far-distant parts, it may be to near ones, or to divert it along collaterals, it may be many, or it may be few, or to spread out at once among numerous seemingly equipollent branches. And whether it prove ultimately true or no that the figure of the dancing molecules sweeps always onwards along the dendrites towards the nucleus, and always outwards away from the nucleus along the neuraxon, or whatever way in the end be shown to be the exact differences in nature and action between the dendrites and the neuraxon, this at least seems sure, that cell plays upon cell only by such a kind of contact as seems to afford an opportunity for change in the figure of the dance, that is to say, in the nature of the impulse, and that in at least the ordinary play it is the terminal of the neuraxon (either of the main core or a collateral) of one cell which touches with a vibrating touch the dendrite or the body of some other cell. We can thus, I say, by the almost magic use of a silver token—I say magic use, for he who for the first time is shown a Golgi preparation is amazed to learn that it is such a sprawling thing as he sees before him which teaches so much, and yet when he comes to use it acquires daily increased confidence in its worth—it is by the use of such a silver token that we have been able to unravel so much of the intricate tangle of the possible paths of nervous impulses. By themselves, the acquisition of a set of pictures of such black lines would be of little value. But, and this I venture to think is the important point, to a most remarkable extent, and with noteworthy rapidity, the histological results thus arrived at, aided by analogous results reached by the degeneration method, especially by the newer method of Marchi, have confirmed or at times extended and corrected the teachings of experimental investigation and clinical observation. It is this which gives strength to our present position; we are attacking our problems along two independent lines. On the one hand we are tracing out anatomical paths, and laying bare the joints of histological machinery; on the other hand, beginning with the phenomena, and analyzing the manifestations of disorder, whether of our own making or no, as well as of order, we are striving to delineate the machinery by help of its action. When the results of the two methods coincide, we may be confident that we are on the road of all truth; when they disagree, the very disagreement serves as the starting-point for fresh inquiries along the one line or the other.

Fruitful as have been the labors of the past dozen years, we may rightly consider them as but the earnest of that which is to come; and those of us who are far down on the slope of life may wistfully look forward to the next meeting of the Association on these Western shores, wondering what marvels will then be told.

Physiology, even in the narrower sense to which, by emphasis on the wavering barrier which parts the animal from the plant, it is restricted in this Section, deals

with many kinds of being, and with many things in each. But, somewhat as man, in one aspect a tiny fragment of the world, still more of the universe, in another aspect looms so great as to overshadow everything else, so the nervous system, seen from one point of view, is no more than a mere part of the whole organism, but, seen from another point of view, seems by its importance to swallow up all the rest. As man is apt to look upon all other things as mainly subserving his interests and purposes, so the physiologist, but with more justice, may regard all the rest of the body as mainly subserving the welfare of the nervous system; and, as man was created last, so our natural knowledge of the working of that nervous system has been the latest in its growth. But, if there be any truth in what I have urged today, we are witnessing a growth which promises to be as rapid as it has seemed to be delayed. Little spirit of prophecy is needed to foretell that in the not so distant future the teacher of physiology will hurry over the themes on which he now dwells so long, in order that he may have time to expound the most important of all the truths which he has to tell, those which have to do with the manifold workings of the brain.

And I will be here so bold as to dare to point out that this development of his science must, in the times to come, influence the attitude of the physiologist towards the world, and ought to influence the attitude of the world towards him. I imagine that if a plebiscite, limited even to instructed, I might almost say scientific, men, were taken at the present moment, it would be found that the most prevalent conception of physiology is that it is a something which is in some way an appendage to the art of medicine. That physiology is, and always must be, the basis of the science of healing, is so much a truism that I would not venture to repeat it here were it not that some of those enemies, alike to science and humanity, who are at times called anti-vivisectionists, and whose zeal often outruns, not only discretion, but even truth, have quite recently asserted that I think otherwise. Should such a hallucination ever threaten to possess me, I should only have to turn to the little we yet know of the physiology of the nervous system and remind myself how great a help the results of pure physiological curiosity—I repeat the words, pure physiological curiosity, for curiosity is the mother of science—have been, alike to the surgeon and the physician, in the treatment of those in some way most afflicting maladies, the diseases of the nervous system. No, physiology is, and always must be, the basis of the science of healing; but it is something more. When physiology is dealing with those parts of the body which we call muscular, vascular, glandular tissues and the like, rightly handled she points out the way not only to mend that which is hurt, to repair the damages of bad usage and disease, but so to train the growing tissues and to guide the grown ones as that the best use may be made of them for the purposes of life. She not only heals, she governs and educates. Nor does she do otherwise when she comes to deal with the nervous tissues. Nay, it is the very prerogative of these nervous tissues that their life is above that of all the other tissues, contingent on the environment and susceptible of education. If increasing knowledge gives us increasing power so to mould a muscular fibre that it shall play to the best the part which it has to play in life, the little knowledge we at present possess gives us at least much confidence in a coming far greater power over the nerve cell. This is not the place to plunge into the deep waters of the relation which the body bears to the mind; but this at least stares us in the face, that changes in

what we call the body bring about changes in what we call the mind. When we alter the one, we alter the other. If, as the whole past history of our science leads us to expect, in the coming years a clearer and deeper insight into the nature and condition of that molecular dance which is to us the material token of nervous action, and a fuller, exacter knowledge of the laws which govern the sweep of nervous impulses along fibre and cell, give us wider and directer command over the moulding of the growing nervous mechanism and the maintenance and regulation of the grown one, then assuredly physiology will take its place as a judge of appeal in questions not only of the body, but of the mind; it will raise its voice not in the hospital and consulting-room only, but also in the senate and the school.

One word more. We physiologists are sorely tempted towards self-righteousness, for we enjoy that blessedness which comes when men revile you and persecute you and say all manner of evil against you falsely. In the mother-country our hands are tied by an Act which was defined by one of the highest legal authorities as a 'penal' Act; and though with us, as with others, difficulties may have awakened activity, our science suffers from the action of the State. And some there are who would go still farther than the State has gone, though that is far, who would take from us even that which we have, and bid us make bricks wholly without straw. To go back is always a hard thing, and we in England can hardly look to any great betterment for at least many years to come. But unless what I have ventured to put before you to-day be a mocking phantasm, unworthy of this great Association and this great occasion, England in this respect at least offers an example to be shunned alike by her offspring and her fellows.

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CHEMISTRY AT THE BRITISH ASSOCIATION.

The work of the Chemical Section of the British Association was inaugurated with the address of its President, Professor Ramsay: 'An Undiscovered Gas.' Starting with a discussion of the history of the various periodic relationships which have been shown to exist among the elements, and of the definition of the properties of unknown members of several of the Groups prior to their isolation, the attempt was made to establish the probability of the existence, and to prophesy the characteristics of an element, as yet unknown, forming a 'triad' with helium and argon. Between fluorine and manganese lies chlorine; between oxygen and chromium, sulphur; between nitrogen and vanadium, phosphorus; between carbon and titanium, silicon, etc. The intermediate element possesses an atomic weight greater, on the average, by 16 units than that of the lightest member of the triad, and less by 20 units than that of the heaviest. Between the lightest and the heaviest, therefore, the difference in atomic weight is approximately 36 units, which is also the difference between the accepted atomic weights of helium (4) and argon (40). "There should, therefore, be an undiscovered element between helium and argon, with an atomic weight 16 units higher than that of helium, and 20 units lower than that of argon, namely 20. And if this unknown element, like helium and argon, should prove to consist of monatomic molecules, then its density should be half its atomic weight, 10. And, pushing the analogy still farther, it is to be expected that this element should be as indifferent to union with other elements as the two allied elements."

Professor Ramsay next reviewed his various efforts to obtain the third member of the helium-argon triad. The most promising method—that of systematic diffusion of the individual gases—failed to show the